Daily Mean Chemical Intensity.

1866.	Kew.	Pará.	Ratio.
April 6	28·6	242·0	8·46
„ 7	7·7	301·0	39·09
„ 9	5·9	326·4	55·25
„ 11	25·4	233·2	9·18
„ 20	38·9	385·0	9·90
„ 24	83·6	362·7	4·34

The measurements were made at Pará in the middle of the rainy season, and at very frequent intervals during the day; the curves show the enormous and rapid variation in intensity from hour to hour which the chemically active rays undergo under a tropical sun during the rainy season.

IX. "On the Elimination of Nitrogen during Rest and Exercise on a regulated Diet of Nitrogen." By E. A. Parkes, M.D., F.R.S. Received June 1, 1867.

The experiments recorded in this paper are intended to complete the inquiry into the effect of rest and exercise on the elimination of nitrogen recorded in the Proceedings of the Royal Society (No. 89, 1867).

The experiments were made on two soldiers at the Royal Victoria Hospital at Netley. One of them (S.) was the subject of the former experiments, the other man (B.) was a fresh man. B. is a perfectly healthy temperate man, aged 22½ years, 5 feet 9¼ inches in height, and weighing 140 lbs.

Extreme care was taken to ensure the greatest accuracy both as to food and as to the collection of the excreta. The whole value of such experiments as these, depends on the exactness with which all the conditions are carried out; and without perfect accuracy, the results would only mislead. I have every confidence that the conditions were faithfully observed; there is in fact evidence of this from the experiments themselves.

The course of the experiments was precisely the same as in the observations recorded in the last paper, except that the diet was during sixteen days exactly the same on each day. During four days the men were at their ordinary employment; during two days rested; returned to ordinary work for four days; took very active exercise for two days; and were then for four days more on ordinary occupation.

They took each day the same amount of food, viz.:—

Articles.	Amount, in ounces av.	Total nitrogen in each article, in grains*.
Bread	16	60·99
Meat (cooked)	9 (15 raw)	213
Potatoes (cooked)	12	12
Cabbage (cooked)	3	·1
Milk	6	16·5
Sugar	3	
Butter....................................	1	?
Salt.......................................	·25	
Infusion of tea	20	?
Infusion of coffee	20	?
Water....................................	3 to 9	
		302·59 or 19·61 grammes.

The bread was made always in the same way; the meat (steak) was of an uniform quality, and was carefully selected every day. The whole quantity of food was regularly eaten and at the same time. The only variation was that the potatoes weighed sometimes 12 or 12½, and sometimes 13 ounces (which, however, made very little change in the nitrogen), and that the amount of water drunk, usually 5 ounces at dinner and 2 at supper (on eleven days), was on five days taken in less quantity. No alcoholic liquid was taken, and there was no smoking.

This quantity of food was just sufficient to preserve the body at almost precisely the same weight; the men were in perfect health.

During the sixteen days 313·76 grammes (viz. $19·61 \times 16$) of nitrogen were known to be taken by each man in the food. The following amounts were recovered from the urine in the same time.

S. .... 303·660 grammes, or 18·97 grammes daily.
B. .... 307·257 grammes, or 19·2 grammes daily.

The nitrogen in the stools (as presently noted) quite made up the difference (10 and 6 grammes) between these numbers and the amount of nitrogen passing in, indeed it rather exceeded it if the average of three days can be applied to sixteen. S. passed regularly rather more nitrogen by the bowels than B., and rather less by the urine.

The weight of the body at the beginning and end was nearly the same, and it is therefore certain that during the sixteen days no nitrogen escaped by the skin or lungs, but that all passed by the kidneys and bowels.

The urine was collected from 8 A.M. to 8 A.M., except on the second days of rest and exercise, when it was collected from 8 A.M. to 8 P.M., and from 8 P.M. to 8 A.M. The nitrogen was determined by soda-lime,

* The nitrogen in the crust and crumb of bread and in the meat was determined once; the other amounts were calculated.

the urea by Liebig's mercuric nitrate, the chloride of sodium being got rid of. The stools were weighed every day*.

On the days of rest the men remained in one room, sitting quite still or lying on the bed; they did not leave the room.

On the first day of exercise they walked twenty-four miles on level ground between 8.10 A.M. and 8 P.M. On the second day they walked thirty-five miles between 8.10 A.M. and 9.45 P.M.

The walking was done well, and S., who had been the subject of the previous experiments of exercise on a non-nitrogenous diet, was quite certain that he supported the fatigue much better under the meat diet, than on the former occasion when he was fed on starch and butter.

The amount of work done (the weight of their clothes being taken into account) was calculated by Mr. Haughton's formula, viz., that walking on a level surface is equal to lifting $\frac{1}{20}$ of the weight through the distance walked.

	First day.		Second day.	
	Kilogramme-metres.	Tons lifted a foot.	Kilogramme-metres.	Tons lifted a foot.
S.	129198=	416	194798=	627
B.	125120=	403	188605=	607

The weight of the body.

The following Table gives the daily variations in the weight of the body during the whole period. The weight was taken at the end of the 24 hours.

	S.	B.
	lbs. av.	lbs. av.
Ordinary occupation, 1st day	145	139·5
,, ,, 2nd ,, ...	145	140
,, ,, 3rd ,, ...	146	139
,, ,, 4th ,, ...	145	139·5
Rest .... 5th ,, ...	144	138·75
,, ... 6th ,, ...	143·5	138·5
Ordinary occupation, 7th ,, ...	144	138·5
,, ,, 8th ,, ...	145	139·25
,, ,, 9th ,, ...	145·5	139·5
,, ,, 10th ,, ...	145	140
Exercise .... 11th ,, ...	142	137
,, ... 12th ,, ...	139·5	135
Ordinary occupation, 13th ,, ...	142	135·75
,, ,, 14th ,, ...	143	137·5
,, ,, 15th ,, ...	144	138·25
,, ,, 16th ,, ...	144·5	139·75

The following Table shows the gain or loss of body-weight in grammes (round numbers).

* I determined also the chloride of sodium and the phosphoric acid (on three occasions), but I have not included these results, in order not to complicate the statement.

	S.	B.
Ordinary occupation, 1st day	0	0
" " 2nd "	0	+ 200
" " 3rd "	+ 400	− 500
" " 4th "	− 400	+ 300
Rest .... 5th "	− 500	− 400
" .... 6th "	− 200	− 100
Ordinary occupation, 7th "	+ 200	0
" " 8th "	+ 500	+ 400
" " 9th "	+ 200	+ 100
" " 10th "	− 200	+ 200
Exercise .... 11th "	−1500	−1400
" .... 12th "	−1000	−1000
Ordinary occupation, 13th "	+1000	+ 500
" " 14th "	+ 600	+ 800
" " 15th "	+ 400	+ 400
" " 16th "	+ 300	+ 600

The weight in the first period was fairly constant; but during the rest-period one man lost 1½ lb. and one other 1 lb. in weight; the loss was gradual on the two days, which was different from the alternations which had gone before; the loss was subsequently recovered from at the rate of rather less than ½ lb. a day until the usual weight was regained on the third and fourth days. As the amount of food ingesta was not less, the loss must have been owing to increase in the egesta. This was certainly an unexpected result, but is yet quite certain.

The nature of the increase in the egesta will appear presently. I will merely state here that it was not owing to any condition of external temperature or atmospheric humidity acting on the skin or lungs. In the first four days of ordinary occupation the maximum temperature in the shade was 59°, 61°·2, 64°·8, and 65° F., while the mean of the maximum and minimum temperatures of twenty-four hours was 51°·2, 52°·6, 50°·2, 54°·4. In the rest-period of two days the maximum shade temperature was 64° and 68°, and the mean temperature of the days was 54°·5 and 58°4. In the after-rest period, when the body was regaining weight on the same diet, the temperature rose greatly, the maximum being 74°·8, 81°·6, 75°, and 70°, while the mean of the maximum and minimum was 61°, 66°·3, 62°, and 59°·5.

It is evident therefore that the weight altered independently of the external temperature; for there was scarcely any difference between the first and rest-period, and if any action had been caused it should have gone on in the succeeding hotter days of ordinary exercise during the third period. The air was a little drier during the two days of rest (65·3 per cent. of total humidity) than in the preceding and following periods (72·6 and 72·9); but this slight difference had no effect, because on one of the days following the rest the air was both hotter and drier than on one of the rest-days, and yet the body gained weight.

During the period of exercise both men lost greatly and almost equally in weight, and then during the following period regained it, so that in four days one man had recovered his former weight, and the other man was only ½ lb. short.

Excretion of Nitrogen by the Urine.

It will facilitate comparison to give the whole of the results in one Table.

Excretion of Urinary Nitrogen, in grammes. First Period.—Ordinary occupation.

Date.	S.						B.					
May 1867.	Quantity of urine, in cubic centimetres.	Urea.	Nitrogen in urea.	Non-ureal nitrogen.	Total nitrogen.	Proportion of ureal to non-ureal nitrogen.	Quantity of urine, in cubic centimetres.	Urea.	Nitrogen in urea.	Non-ureal nitrogen.	Total nitrogen.	Proportion of ureal to non-ureal nitrogen.
1st day	1460	37·668	17·578	·308	17·886		1130	41·245	19·247	1·170	20·417	
2nd „	1210	35·695	16·657	·153	16·810		810	34·587	16·140	1·378	17·518	
3rd „	1210	36·300	16·940	2·272	19·212		810	34·425	16·065	1·025	17·090	
4th „	1205	37·355	17·432	·088	17·520		870	38·280	17·864	1·119	18·983	
Mean	1271	36·754	17·1517	·7052	17·857	1 to ·041	905	37·134	17·329	1·173	18·502	1 to ·067

Second Period.—Rest.

Date.	S. Quantity of urine, in cubic centimetres.	Urea.	Nitrogen in urea.	Non-ureal nitrogen.	Total nitrogen.	Proportion of ureal to non-ureal nitrogen.	B. Quantity of urine, in cubic centimetres.	Urea.	Nitrogen in urea.	Non-ureal nitrogen.	Total nitrogen.	Proportion of ureal to non-ureal nitrogen.
5th day	1250	39·75	18·550	1·544	20·104		1400	40·6	18·946	1·175	20·121	
6th „ 8 a.m. to 8 p.m.	604	19·932	9·302	·553	9·855		800	22	10·266	1·012	11·278	
6th day 8 p.m. to 8 a.m.	540	17·01	7·938	·377	8·315		520	15·6	7·280	·264	7·544	
Mean of 2 days	1197	38·346	17·895	1·237	19·137	1 to ·079	1360	39·1	18·246	1·225	19·471	1 to ·067

Third Period.—Ordinary occupation.

7th day.........	920	34·04	15·885	·035	15·920		750	34·5	16·1	·582	16·682	
8th ,, ...	960	37·44	17·372	·236	17·608		800	39·2	18·283	·332	18·615	
9th ,, ...	1180	38·94	18·172	1·210	19·382		920	41·4	19·32	1·262	20·582	
10th ,, ...	960	34·08	15·914	1·626	17·54		910	35·035	16·349	1·712	18·061	
Mean	1005	36·125	16·836	·7767	17·612	1 to ·046	845	37·534	17·512	·972	18·485	1 to ·055

Fourth Period.—Exercise.

11th day.........	1000	35·5	16·566	1·912	18·478		1110	39·96	18·648	·342	18·99	
12th ,, 8 a.m. to 8 p.m.	430	15·05	7·023	·324	7·357		565	19·492	9·096	·957	10·053	
12th day 8 p.m. to 8 a.m.	650	26·741	12·479	·978	13·457		540	21·600	10·080	·795	10·875	
Mean of two days	1040	38·645	18·034	1·607	19·646	1 to ·089	1107·5	40·526	18·912	1·047	19·959	1 to ·055

Fifth Period.—Ordinary occupation.

13th day.........	900	43·65	20·370	·88	21·25		1000	38	17·734	2·517	20·25	
14th ,, ...	1000	39·5	18·433	1·509	19·942		1100	40·15	18·736	·537	19·273	
15th ,, ...	1430	42·9	20·029	3·459	23·488		1250	35·625	16·625	2·623	19·248	
16th ,, ...	1730	37·195	17·357	2·179	19·536		1610	41·86	19·534	2·063	21·597	
Mean	1265	40·811	19·047	2·006	21·054	1 to ·105	1240	38·909	18·157	1·935	20·092	1 to ·106

The elimination of nitrogen by the urine followed precisely the same course in each man; and allowance being made for the difference in food, this course was identical with that determined in the former experiments, when the diet was non-nitrogenous. It is certain that neither during rest nor exercise did nitrogen pass off by the skin or lungs.

It will be convenient to consider the total nitrogen in the first instance.

During the first period of four days the total nitrogen excreted was 71·428 grammes by S. and 74·008 grammes by B. In the period of rest, instead of falling the nitrogen increased in amount, so that in two days 38·274 and 38·943 grammes were excreted. This is not only more than the half of the previous four days, but more than the amount of either the first two or the last two days of the first period. The greatest increase was in the first day of rest, but in the second day the amount was still above the mean of the previous period.

As afterwards shown, this was not owing to lessened elimination by the bowels; for both the weight of the stools and the nitrogen increased in the period of rest. It seems impossible to avoid the conclusion that the condition of rest with an equal entry of nitrogen was accompanied by a daily increase of excretion by the urine of about 1 gramme more nitrogen.

It may, indeed, be said that this is within the limits of error or unavoidable variation, and may be accidental; but if so, it seems most remarkable that the result should run in the same way and be of nearly the same amount in each case, and be confirmed by the independent observation of the urea. In the third period, when the men returned to their ordinary occupation, the nitrogen fell in both on the first day to a lower point than had ever before been noted, and then rose gradually, so that in the four days the amount was almost the same with that of the first period, 70·45 and 73·94 grammes being excreted. In the period of exercise which is to be compared with that of the rest, the results were identical with those of the former experiments when nitrogen was not supplied.

On the first day of exercise the nitrogen in each man fell below the corresponding day of rest by 1·626 and 1·131 gramme. In the next twelve hours, which were almost entirely occupied in exercise, the diminution was still greater, being 2·498 and 1·225 grammes, which would be equivalent to 5 and $2\frac{1}{2}$ grammes for twenty-four hours. In the last twelve hours, of rest after work, the elimination increased greatly, so that 5·142 and 3·331 grammes more were excreted than in the corresponding rest-period; the general result being that on the whole two days' period of exercise, as compared with the whole period of rest, there was an increase of about 1 gramme in the exercise-period in each man, owing entirely to the large excretion in the last twelve hours.

	S.	B.
	grammes.	grammes.
Total nitrogen in urine in two days' rest ...	38·274	38·943
Total nitrogen in two days' exercise	39·292	39·918
	1·018	0·975

The first day following the exercise was a day of almost complete rest; the nitrogen in both men was increased considerably over the average of the first and third periods, and very greatly indeed over the amount of the first day of the third period, the excess being 5·33 and 3·568 grammes over that day. This was the most considerable variation in the period of experiment. The nitrogen continued high all through this period, the result being that in the four days S. excreted 84·216 and B. 80·368 grammes, or 13 and 6 grammes respectively in excess over the first period of four days. It is clear indeed that during this period, the excretion of nitrogen must have been greater than the ingress.

I will not trace the changes in the urea in such detail. They were almost identical with those in the total nitrogen.

In the first period the amount of urea was almost precisely the same in the two men. In the rest-period it increased nearly 2 grammes daily in each man, fell during the third period to the former average, decreased greatly during the first thirty-six hours of the exercise-period as compared with the rest-period, and increased in the last twelve hours; in the last or after-work period it also increased, though in a less proportion than the total nitrogen.

The changes in the non-ureal nitrogen were also very similar in the two men, but will be best followed in the case of B., in whom the excretion of non-ureal nitrogen was more steady from day to day than in S. It was very slightly and immaterially increased in the rest-period, fell as slightly in the after rest-period, remained the same during the exercise-period, and increased to nearly double in the last four days. In S. it increased more in the rest-period and in the exercise-period than in B., and still more in the last four days. This increase in the non-ureal nitrogen after exercise is confirmatory of the results formerly obtained on this point.

If these results are looked at as a whole, it will be seen that though the changes in the amount of nitrogen were for the most part not great, still they were decided and evident changes, and occurred precisely in the same way in the two men. The coincidence in the changes in the urea and in the total nitrogen (determined by such different processes) is a strong argument that the results were real. Throughout the whole time the food was precisely the same, and the modifications were therefore not owing to variation in the ingress of nitrogen.

There was some variation in the amount of urinary water; but the

increased excretion of nitrogen was, I believe, not at all connected with it. Thus in the first and third periods the nitrogen was almost the same, yet in S. the difference in the mean amount of water was 266 cub. centims., and in B. was 60 cub. centims. In S., in the fifth period, the amount of water was the same (within 6 cub. centims.) as in the first period, yet the nitrogen was more than 3 grammes in excess. If individual days are taken, no obvious relation appears between the urinary water and the nitrogen. The largest amount of water in S. (1760 cub. centims.) corresponded to 19·536 nitrogen, while the largest amount of nitrogen (23·488) corresponded to 1430 cub. centims., and the next amount of nitrogen (21·25) was passed in only 900 cub. centims. of urine. In B. the largest amount of nitrogen (21·597) was contained in the largest amount of water (1610 cub. centims.), but almost as great an amount was contained in 1000 and 920 cub. centims. So that differences in the amount of water cannot explain the variations in the exit of nitrogen. If not owing to alteration in food, nor to variable passage of water through the kidneys, it seems tolerably certain that the conditions of rest and exercise were the causes of the variation.

Excretion of Nitrogen by the bowels.

The two men did not have quite the same amount of intestinal excreta. The average daily weight (sixteen days) in the case of S. was 4·798 ounces avoirdupois or 136 grammes; while in the case of B. they amounted only to 3·97 ounces, or 112·8 grammes.

The exact daily weights are given further on, and I will now merely state the amount of nitrogen, which was determined three times.

	Nitrogen in grammes.	
	S.	B.
Last day of first period...............	1·227	0·644
Last day of rest........................	1·486	1·091
Last day of exercise	2·138	1·504
Mean	1·617	1·079

B. passed (if these three days represent the mean) 0·538 gramme less nitrogen daily by the bowels than S., and during the first twelve days he passed on an average 0·6 gramme more nitrogen in the urine, so that during these twelve days the discharge of nitrogen by the conjoint channels was within 1 gramme the same in the two men; the amounts being in S. 238·848, and in B. 239·757 grammes in twelve days, while the amount of nitrogen passing in (independent of a small amount in the tea, coffee, butter, &c.) was 235·32 grammes. This accordant result proves, I believe, both the estimate of the nitrogen in the food and the collection and analysis of the excreta to have been accurate. I was

quite unprepared for a result so close as that the difference in the excretion of nitrogen of the two men should be only 0·076 gramme, or scarcely more than 1 grain daily. In the last four days S. passed a little more nitrogen by the urine than B., thereby reversing what had gone before. The stools were not analyzed during this period, but I believe that the nitrogen must have been furnished by the body during these four days. As respects the effect of exercise on the intestinal nitrogen, there was a slight increase in rest over the previous period and in exercise over the rest-period.

If the following Table (p. 54) be analyzed, it will be found that the loss of weight in the rest-period was attributable in S. almost entirely to excess in the pulmonary and cutaneous excreta, while in B. it was owing to increase in the urinary and intestinal excreta. It might be presumed to have been chiefly water; but the simultaneous changes in the excretion of nitrogen give it interest. The channel of elimination in B. proves in another way that it was not owing to effect of external temperature in the air.

During the period of exertion the loss of weight was from increase in the skin and lung excretion, and it is interesting to observe how parallel it was in the two men; the loss of weight was subsequently made up by lessening of the skin and lung excreta. The intestinal excreta were not influenced either way by the exercise; and in spite of the great passage of water by the skin, the urinary water was not affected. The antagonism commonly stated to exist between the excretion of water by the skin and kidneys was not perceptible.

Explanation of the preceding facts.

Taking into account the experiments formerly recorded as well as those in this paper, we have to explain the following phenomena.

1. With an unchanged ingress of nitrogen there was a slight excess of nitrogenous excretion during rest as compared with a period of ordinary exercise.

2. There was a decrease of urinary nitrogenous excretion during active exercise as compared with a period of rest, and this was perceptible both when the ingress of nitrogen was stopped, as well as when nitrogen was supplied in regular amount.

3. There was an excess, not great, but long continued in nitrogenous excretion after exercise.

4. There was a retention of nitrogen in the system when it was again supplied after having been cut off, after both rest and exercise, and greatest in the latter case, showing that it is needed in the system, and that an insufficient supply at one time must be subsequently compensated.

In addition we cannot leave out of account the well-known dietetic fact, based on experience, that much muscular work always demands the supply of a larger amount of nitrogen.

TABLE showing the daily weights in grammes of the excreta.—The urinary and intestinal excreta were measured and weighed; the pulmonary and cutaneous[*] excreta were determined by calculation, the ingesta, the changes in body-weight, and the weight of the urine and fæces, furnishing the elements of the calculation. The atmospheric oxygen was disregarded.

		S. Egesta.				B. Egesta.		
	Food ingesta.	Urinary.	Intestinal.	Pulmonary and cutaneous.	Food ingesta.	Urinary.	Intestinal.	Pulmonary and cutaneous.
Ordinary.								
During 1st day	2783	1510	106·5	1166·5	2669	1180	106·5	1382·5
" 2nd "	2683·8	1256	220	1207·8	2559	850	142	1367
" 3rd "	2648	1255	106·5	886	2641	851	92·3	2197·7
" 4th "	2733	1251	99·4	1782·6	2733	916	59·6	1457·4
Rest.								
During 5th day	2698	1287	198	1589	2690	1448	149·1	1493
" 6th "	2726	1185	106·5	1634·5	2712	1346	142	1324
Ordinary.								
During 7th day	2740	960	99·4	1481	2740	782	92·6	1465
" 8th "	2740	1005	227	1008	2726	847	177·5	1601·5
" 9th "	2753	1228	179	1146	2726	970	89·4	1466·6
" 10th "	2726	1001	116	1309	2714	964	149·1	1501
Exercise.								
During 11th day	2754·8	1043	106·5	3105	2804	1161	106·5	2937
" 12th "	2811·6	1079	144	2588·6	2740	1155	95·1	2490
Ordinary.								
During 13th day	2726	947	99·4	679·6	2726	1044	113·6	1068·4
" 14th "	2726	1048	134·9	943	2733	1147	92·3	694
" 15th "	2733	1483	149·1	700·9	2747	1293	78·1	976
" 16th "	2740	1766	85·2	677	2726	1666	120·7	339

Both the theories of muscular action now being discussed by physiologists seem to me insufficient to account satisfactorily for all the above facts.

The old theory was, that a muscle was more or less destroyed during action and was repaired during rest, and if so, it seemed reasonable to suppose that the action of the muscles would be measured by the amount of nitrogen eliminated. But the decrease in the nitrogenous excretion during exercise and its very moderate increase afterwards (an increase quite out of proportion to the amount of muscle supposed to be destroyed) seem quite inconsistent with this view.

The new theory, springing from the experiments of Professors Fick and Wislicenus, viz., that the nitrogenous framework of a muscle is merely the machinery which allows changes in the non-nitrogenous substances to take place, and that in itself it undergoes during exercise no change, though at first sight consistent with some of the facts, does not appear to be so with all. It does not account for the increase of nitrogenous excretion in rest, for the decrease during exertion, or for the increase afterwards, nor in a satisfactory manner for the great retention of nitrogen in the system which occurs after exercise on a non-nitrogenous diet.

There is something more in the facts than either disintegration *per se*, or stability of nitrogenous composition during muscular action, will account for.

We must find some other explanation; and it appears to me that we can only express the facts by saying that a muscle during action appropriates more nitrogen than it gives off, and during rest gives off more than it appropriates. We have, perhaps, strictly speaking, no right to go beyond this; but it seems clear that as a muscle could hardly be supposed to have two simultaneous actions, we may simplify the above expression by stating that during action a muscle takes nitrogen, and during rest gives it off. To put this in other words, the action of a muscle would seem from these experiments not to be connected with disintegration, but with formation; when it is in exercise the muscle increases, when it is quiescent it lessens in bulk. It may seem a bold innovation to attempt to reverse in this way the old theory of muscular action, especially as the same rule would have to be applied to nutrition generally; but if it explains all the facts, it is at any rate entitled to be fully considered.

In applying this expression in the explanation of the facts, I must premise that the nitrogen discharged by the kidneys and bowels cannot be supposed to be derived solely from the muscles. As it represents all the nitrogen going in, it must be derived from all the nitrogenous tissues, from the nervous substance, the gland cells, the albuminous membranes and fluids, in fact from all nitrogenous structures. That portion of it which is derived from the muscular system comes only in part from those

muscles whose state we can alter. We cannot alter the action of the muscles of respiration, of the heart, the stomach, and intestines, &c. We cannot even reduce the voluntary muscles to a state of complete and prolonged rest. There must be some movement, consequently we must not expect to find large variations in the elimination of nitrogen when a certain number of muscles only are kept in a state of comparative rest or exercise.

The food passing into the body after due preparation in the stomach, liver, and lungs, forms in the blood a reservoir or store of nutriment from which the different parts of the body take their supply as they require it, or according as the special stimulus of each enables it to appropriate it.

In these two men 19·6 grammes, or 302 grains passed daily into, and then out of, the store into the various nitrogenous tissues. This quantity exactly sufficed in the then state of activity of all the organs to preserve perfect action, and to keep the body-weight constant.

A certain number of muscles being brought into a state of rest, the nitrogenous elimination increased; in other words, the muscles appropriated nitrogen in less, and gave it off in greater, amount, owing, if my explanation be correct, to their more rapid disintegration during rest than exercise. This may be understood by supposing that if in the twenty-four hours the voluntary muscles are in a state of rest for twelve, and of exercise for twelve hours, and if the exercise is reduced to six hours, the removal going on at the same rate for eighteen hours instead of twelve hours will increase the exit of nitrogen 50 per cent. Accordingly during the period of rest the elimination of nitrogen increased, and this was necessarily most marked during the first day, when the bulk of the quiescent muscles was greater than on the second day, when it had been reduced by excess of elimination. I do not see how properly to explain the increase during rest except in this way; if the fact be as I state it, no theory of muscular action can be true which does not account for it.

The effect on the reserve or store of nitrogen in the blood would be to leave in it more nitrogen than usual at the end of the two days' rest. The men then commenced ordinary occupation, and immediately the muscles began again to contract and to assume more nitrogen in consequence of the increased exercise. As they had to regain their former composition, the elimination of nitrogen necessarily lessened, and the reserve must have fallen to its normal amount. They would use up the accumulation in the reserve as well as the fresh supply, and the equilibrium would be restored; this was nearly done in fact in twenty-four hours, as may be seen in the Table.

After four days the men took excess of exercise. The elimination of nitrogen at once lessened, because more was used by the contracting muscles, and there were lesser intervals of rest.

The last $10\frac{1}{2}$ hours of the two exercise days formed a period of rest; and during this time the excretion increased, and this increase continued more or less for four subsequent days.

This might be explained by the passing off of excretory products formed during the contraction, according to the old theory; but if so, it seems singular that the increased excretion should have been so moderate, and at the same time should have been spread over so many days, whereas on the hypothesis I have suggested it is easily explicable.

During the exercise-period the extra action of the muscles had added a large amount of nitrogen to their structure; at the end of the time the muscles must have been bulkier, and therefore in the succeeding period of rest furnished a larger elimination of nitrogen than in the after rest-period when they were smaller. Moreover, after the exercise-period there was much more rest than after the rest-period. In the first day after the exercise the men were tired and rested the whole day, and even on the following days did not probably make so much exertion as usual. And the gradual elimination for so many days looks much more like a temporarily enlarged organ returning slowly to its normal size, than like the passage of accumulated excretory products; the chief product being the very soluble urea which is always so rapidly removed from the muscles that scarcely any can be detected in them.

The facts observed in the experiments on a non-nitrogenous diet seem now to be also easily explained. The decrease in the urea during the period of exercise equally occurred, because the muscles used more nitrogen in their action than in the rest-period, taking it from the store, and thereby no doubt robbing other parts.

During the two days of exercise without nitrogen, the muscles may have been just as well fed with nitrogen as during the experiments with 300 grains, only other parts could not have been so; other organs and the muscles not called into play must have acquired nitrogen with more difficulty, and consequently when nitrogen was again given, a large portion was retained to replenish the store and to feed the organs which had been on short allowance.

The quantity retained when nitrogen was again given did not serve (we may suppose) to nourish muscles exhausted by the exercise (which on my theory had even increased in nitrogenous constituents), but other parts.

If this reading of the facts be admitted, it may be asked how it will affect the inference drawn from the experiments of Professors Fick and Wislicenus. They determined the nitrogen discharged, calculated how much muscle it represented, and then argued (and as Dr. Frankland has shown, correctly argued) that this amount of muscle could not have produced the mechanical force which had been exerted. But it is apparent, if I am correct, that the measure of the work must be the amount of nitrogen appropriated by, and not that eliminated from, the muscle, and this was not shown in their celebrated experiments.

But though doubt may be raised as to the basis of their opinion, I

conceive the opinion itself was probably correct. Because even if the work is done during the period when nitrogen is added, and not when it is eliminated, it is difficult to suppose that the changes in the nitrogen are on a scale large enough to account for the result, or that the transformation of a particle of blood-albumen into a particle of muscle-albumen could be attended by any chemical changes which *per se* could equal the mechanical force produced. But we can imagine that such a transformation may be the cause of changes in the non-nitrogenous substances to which the manifestation of force is really owing. There is no reason why disintegration should be more attended with such changes than formation. Indeed it is perhaps more often that the union of chemical substances is attended by signs of transformation of force than their disunion. Or the stimulus which causes the addition of the nitrogen to the muscle may at the same moment originate the changes in the non-nitrogenous substances.

The fact that the substances the presence of which in the muscle suspends the contraction (and therefore, if I am right, the growth of muscle), appear from Ranke's latest observations to be derived from the non-nitrogenous substances, is another argument in favour of the view that great changes go on in these substances during muscular action.

If the opinion of Professors Fick and Wislicenus to this extent, and if the experiments of Ranke and others on the effect of the effete products be adopted, the following would be the theory of muscular action I would propose.

When a voluntary muscle is brought into action by the influence of the will, it appropriates nitrogen and grows; the stimulus or the act of union gives rise to changes in the non-nitrogenous substances surrounding the ultimate elements of the muscular substance which cause the conversion of heat into motion. The contraction continues (the will still acting) until the effete products of these changes arrest it; a state of rest ensues, during which time the effete products are removed, the muscle loses nitrogen, and can again be called into action by its stimulus.

This theory not only explains the experiments now recorded, but simplifies our ideas both of the growth and of the wasting of muscle, and seems likely to explain more easily some processes in disease.

It is also in greater accordance with the rules of diet derived from experience than the theory of Fick and Wislicenus. If correct, it shows why the muscle requires nitrogen for its action, and why increased action requires increased nitrogen. The food must either supply this, or the store of nitrogen in the blood and other organs must be lessened*.

* That an increased supply of fat, and perhaps of starches, is also desirable has long been practically recognized, though the store of fat already in the body renders this less necessary for a time. The observations of Lawes and Gilbert seem to me to render it possible that when a muscle parts with its nitrogen, fat is formed, and if so, a muscle disintegrating during rest may form a store of fat in its texture which may be further transformed at the next addition of nitrogen, *i.e.* at the next contraction.

It enables us to understand why in a well-fed body it may be some time after nitrogen is cut off before the muscles have any difficulty in obtaining what they want, and why in a body ill-supplied with nitrogen exertion lessens, or if kept up produces bad effects.

If exertion is persevered in under such circumstances, a failure somewhere is always observed. Frequently the nervous system or the heart shows signs of weakness, a result which could hardly be explained by the view of the Swiss Professors. It is certainly an argument for the view I have advocated, that it is in harmony with the teachings of experience, and restores to the rules of diet their old significance.

X. "Note on the Lunar-diurnal Variation of Magnetic Declination." By J. A. Broun, F.R.S. Received May 11, 1867.

Lausanne, 7th May, 1867.

I received late last night No. 91 of the Proceedings of the Royal Society, and desire to offer the following remarks on the abstract of a paper by Mr. Neumayer which I find therein (vol. xv. p. 414).

Mr. Neumayer is evidently unacquainted with the Note by me, read to the Royal Society of London in 1861 (Proc. Roy. Soc. vol. x. p. 475), in which I stated as result of the discussions of five years' observations at Trevandrum (near the magnetic equator) that the lunar-diurnal variation of magnetic declination became inverted, like the solar-diurnal variation, when the sun passed from one hemisphere to the other, both the solar- and lunar-diurnal variations depending on the position of the sun.

I also stated the laws of the lunar-diurnal variation, not only for the moon north and south, as Mr. Neumayer has done, but also for the moon on the equator moving northwards, and again on the equator moving southwards, the laws being different for the moon in the same position according as she was moving in one direction or in the other.

I pointed out in the Transactions of the Royal Society of Edinburgh (vol. xviii. p. 354), that the reversal of movement of the declination-needle with the sun north and south of the equator, observed within the tropics, had its equivalent in the different ranges of the solar-diurnal variation for summer and winter in high latitudes. It followed in like manner that, the lunar-diurnal variation being inverted with the solar-diurnal variation near the equator, a similar difference of ranges should be observed in the laws of lunar-diurnal variation for summer and winter in the higher latitudes. Of this fact I satisfied myself by a rediscussion of the Makerstoun observations, after rejecting the large disturbances.

Another consequence of the law of inversion of the lunar-diurnal variation near the equator with the sun's passage from one hemisphere to another, and with the inversion of the solar-diurnal variation, was the opposition or approximate opposition of the mean curves